correct for A 21. The use of the factor 3 for all patterns of anemometers now for nearly thirty years has introduced a great amount of error in published wind velocities; so that they are not only not comparable generally with one another, but the errors have likewise affected most, if not all, the results obtained from the discussions of these velocities. It is much to be regretted, therefore, that some standard pattern had not been adopted and its constant accurately determined at the start, instead of deferring it for nearly thirty years; for, if this is even now done, it will be a long time before any adopted standard and its true constant can come into general use.

Since the force of the wind is as the square of the velocity, errors in the estimated velocity of the wind give rise to errors in the pressure of the wind which are proportionately more than twice as great. For instance: if the true velocity of the wind is 30 miles per hour, the Kew Standard with its factor 3 makes it 42 miles nearly, an increase in the ratio of 1 to 1.4; but the force of the wind is increased in the ratio of 30^2 to 42^2, or as 1 to 2 nearly, and so in a ratio more than double the preceding one. In estimating the force of the wind from the indications of the anemometer, the effect of the error in the factor 3 of the anemometer, and of the wind-pressure constant .005, now in general use, are both in the same direction; so that the combined errors of both are very great. For instance: in the case of a wind of 30 miles per hour, we have seen above, that the error of the factor 3 applied to the Kew Standard increases the force of the wind in the ratio of 1 to 2; and if the wind-pressure constant should be .003 instead of .005, then the effect of both errors is to increase the estimated force of the wind above the true force in the ratio of 1 to $2\times\frac{5}{3}$, or to more than three times the real force. Of course, this is an extreme, but not an impossible case; for in anemometers mostly used the error of the factor 3 is not nearly so great as for the Kew Standard, and the true value of the wind-pressure constant may come out a little more than .003 when accurately determined, but still the errors of estimated wind forces, with the constants in use, are undoubtedly enormously large. Mr. Whipple, of the wind-force committee, says, that, "unless the Robinson anemometers could be put into the hands of those who would take care of them, their indications were frequently worse than useless. The instruments require to be continually looked after. Even if carefully attended to and regularly cleaned and well oiled, their records are far from satisfactory."

It is the opinion of the writer that they must in time give way to something better, probably to Mr. Dines' newly invented helicoid anemometer, which is more simple in its mechanical action, and, according to the experiments made with it, seems quite satisfactory. A description of this instrument is found in the *Quarterly Journal of the Royal Meteorological Society* for July, 1887.

WM. FERREL.

The Soaring of Birds.

THERE can be no doubt that the explanation of soaring given by Mr. Gilbert is mechanically sound. The only remaining question seems to be as to its sufficiency. In regard to this question, the following considerations may be of service:—

There is a certain velocity relative to the air such that a bird possessing it can be sustained against gravity without muscular exertion. Let V represent that velocity for a given bird. Let there be two horizontal layers of air, whose relative velocity is i. For simplicity, let the velocity of the lower layer be zero, that of the upper i. Suppose the bird at some instant to be in the upper layer, moving in the same direction with it, and with a velocity relative to it of V, so that he can just be sustained while moving horizontally. His velocity relative to the lower layer is $V+i$. Let him now descend into the lower layer and wheel horizontally 180 degrees. In so doing he necessarily loses some energy, and his velocity decreases. Now, in order that he may be sustained at the same level during the wheeling, his velocity relative to the lower layer must not fall below V. Suppose his decrease of velocity to be a little less than i; he will then be moving opposite to the direction of the upper current, with a velocity greater than V. He can therefore not only maintain his level, but can rise. Let him now enter the upper layer, his velocity relative to it being $V+i$. If, now, he can wheel horizontally through 180 degrees without losing more than the velocity i, he will be in a position to repeat the cycle.

The statement of Professor Oliver in *Science* (xiii. p. 16) seems to imply that the difference in velocity of the air-currents needs to be as great as the relative velocity which will enable the bird to sustain himself against gravity; that is, that i must be as great as V. If the discussion here given is correct, such is not the case. It is only necessary that the bird should have initially a sufficient relative velocity, and should be able to wheel horizontally 180 degrees without losing by "friction" enough energy to reduce his velocity as much as i, the velocity of one air-current relative to the other.

L. M. HOSKINS.

University of Wisconsin, Madison, March 5.

"Shall We Teach Geology?"

WHEN a reviewer bases critical verdicts on ignorance or misapprehension of the work reviewed, he has an advantage over the author, of which, in my own experience, I usually leave him in quiet possession. Still the meekness of silence may not always prove most useful to the public. Your reviewer of my work, "Shall We Teach Geology?" in No. 317, says that I ignore the mental and moral sciences as means of culture; but he should have observed that I do not undertake to discuss the education value of all sciences and literatures, but only of those selected as types by certain pedagogical writers who hold geology in disesteem. Your reviewer states that I mention "history only to slight it, declaring that it trains no faculty but verbal memory." My criticisms on history contemplate it as a study urged upon children in the early stages of education. This is what I have recorded on purpose to forestall such an accusation. "My present investigation concerns studies as usually taught and in schools of the lower orders. In college, history and literature are pursued in a nobler and more cultural way" (p. 148). Your reviewer employs the term "literature" in the wide sense, which makes it a much more valuable thing than literature as used in the narrow sense of the author, whose positions I am examining (note, p. 145). Your reviewer states, also, that I claim for geology that "the subject should be taken up in the primary schools, and pursued every year as long as the student attends school." This is preposterous criticism. Such is not my position, nor is the idea anywhere conveyed. I think the subject should be taken up briefly, two, three, or more times, at successive stages of mental development, not completed in one course late in school-life (see pp. 133, 134).

ALEXANDER WINCHELL.

Ann Arbor, Mich., March 5.

To keep Water-Mounts Moist.

IN my last communication on this subject (*Science*, xiii. p. 170) I recommended glass capillary tubes. I since find that a much simpler plan, and one that serves equally well in most cases, is to suspend from the edge of the cover-glass, to a beaker of water beneath, a moistened piece of filter-paper about four centimetres long and half a centimetre wide.

Likewise, in the study of germination of seeds, the capillary tubes or the moistened filter-paper may be put to good service. Very clean and satisfactory specimens of the first stages of germination may be obtained by placing the moistened seeds in contact one with another on a glass slip over a beaker of water, and suspending from their midst to the water one of the tubes or simply a narrow piece of paper. A bell-jar will exclude dust.

E. B. KNERR.

Parsons College, Fairfield, Io., March 6.

The Wind-Pressure Constant.

IN my note I see you have put Hazen for Hagen. The latter is a German physicist of Berlin. Will you please make the correction in your next number? This is important, since Hazen has also made experiments, the results of which differ very much from Hagen's, and it may seem that I have misrepresented his results.

WM. FERREL.

Kansas City, Mo., March 5.